England her letters were restored to her, and in the present volume she has arranged them in chronological order, with extracts from her journal. The book contains a mass of petty details in which few readers will find much to interest them; but there are also some very good sketches of the scenery of Natal and of the rough, free-and-easy life of the colonists. Mrs. Feilden was much impressed by the fertility of the soil, and by the beauty of the vegetation with which she was surrounded. "As for fruits, vegetables, and flowers," she wrote, "you have only to put the seeds and young plants in the ground and they grow. There is no end of season in Natal." She remarked that there were not many native fruits, but that all that were imported seemed to suit the soil and climate. The native flowers she considered "very exquisite." They "grew in great variety and luxuriance, with the waxy look of hot-house plants." As for birds and insects, the air teemed with them. Of the Caffres Mrs. Feilden formed a very poor opinion. "The Caffre is indolent; he lives only like the beast, to eat and sleep, and pass through life with ease; but to do this he must have his land tilled, and to purchase wives to till his land he must have cows to pay for them. He sells his daughters to be drudges to other Caffres, while the boys and young men go out to work for the white man, till they can in turn buy cows and wives." Even Caffres, however, have one good quality: "they heartily share anything they have with each other, and eat out of the same pot without the least feeling of who shall have most." To Mrs. Feilden they seemed to be rather like Jews, and she asks whether they may not be descendants of Ishmael or an offshoot from the lost tribes—from which it may be inferred that in the list of subjects she has tried to study ethnology has not yet been included.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Coal-Dust Theory

UNDER the title of "A New Mining Danger," the *Yorkshire Post* of the 16th ult. published its final report on the proceedings at the inquest on the bodies of twenty-two men and boys who perished in an explosion at Altofts Colliery, Normanton, on the 2nd of October last.

The inquest was commenced immediately after the explosion, but was adjourned until the workings could be sufficiently re-opened to admit of a careful examination being made into all the circumstances. It was resumed and concluded on the 13th, 14th, and 15th of December. The witnesses, including Mr. F. N. Wardell, Her Majesty's Inspector of Mines for the district, were unanimously of opinion that the explosion was originated by the firing of a shot in the stone or rock constituting the side of one of the main thoroughfares of the colliery, which was also a main intake airway; and they were of opinion that it had expended part of its energy in raising and igniting a cloud of coal-dust, which formed the nucleus whence the explosion was propagated.

The coroner in summing up agreed with this conclusion, and the jury returned the following verdict:—

"That the whole of the workmen killed, except Deakin and Lomax, met their deaths from an explosion of coal-dust, which originated in the west chain road, which explosion was caused by the firing of an unskilfully drilled shot by one of the men engaged in widening the road; and that Deakin and Lomax were suffocated by the stoppage of ventilation consequent on the explosion."

The coal-dust theory, which is discussed at some length in the Final Report of the Royal Commissioners on Accidents in Mines, postulates that coal-dust may not only serve to originate an explosion, under certain given conditions, but may continue to carry it on as far as the same conditions extend. The essential conditions appear to be: that the coal-dust be perfectly dry, in a very fine state of division, and fairly abundant in quantity; that the initial cause be a large flame expanding with sufficient force to propel the air rapidly in front of it, so as to raise a cloud of coal-dust; and, lastly, that the explosion take place in a confined space, such as the workings of a mine.

From the first this theory was intended to account for great explosions in mines, or accomplished facts, which seemed to be otherwise inexplicable. It has nothing to do with mines in which explosions have not taken place, except to point out a possible source of danger; and arguments opposed to it which are based upon the fact that all kinds of coal-dust are not equally inflammable are therefore obviously inapplicable. It was adopted by the Prussian Commissioners as being applicable to certain inflammable dusts in a minute state of subdivision, but not to others of a somewhat different chemical composition. In discussing the Camphausen explosion in NATURE (vol. xxxi. p. 13), I pointed out that the conclusions arrived at from a consideration of the experimental results obtained with Camphausen dust by the Prussian Commissioners, fell far short of the actual occurrence in the colliery, and that the same relation would probably obtain in the case of all the other dusts experimented with, provided all were made equally fine.

MM. Mallard and Le Chatelier, whose conclusions were accepted by the French Commission, of which they were members, rejected the coal-dust theory, and endeavoured to reason away all apparently confirmatory arguments drawn from the consideration of actual explosions in mines.

In this country it has been accepted by some of the Inspectors of Mines, and more particularly by Messrs. W. N. and J. B. Atkinson, who uphold it vigorously in their treatise on "Explosions in Mines," which was recently reviewed in NATURE (Nov. 4, 1886, p. 1) by Prof. Thorpe. It appears also to have found favour with a number of mining-engineers and colliery managers, both here and abroad. There seems, however, to be some doubt as to how far it was accepted by the Royal Commissioners on Accidents in Mines, and I shall therefore quote their own words on the subject:—

"In emphasising this claim (Proc. Roy. Soc., vol. xxxvii. p. 43), Mr. Galloway does not appear to have realised the fact that, *if dust were the principal agent* in coal-mine explosions, *every* blown-out shot occurring in a *very* dusty and dry mine should actually be attended by a more or less disastrous explosion or conflagration, and that, looking therefore to the enormous amount of powder expended in shot-firing in this and other countries, and to the not inconsiderable proportion which blown-out shots must constitute, in many localities, of the total number of shots fired, disastrous coal-mine explosions should be of more than daily occurrence, if his view were correct."

I submit that the conclusion here stated is not deducible from the premiss; and in support of this position I adduce the fact that coal-dust is *admitted* to have been *the principal agent* in two of the most disastrous explosions of the present year, namely, those of Mardy and Altofts collieries, in both of which shots were fired. Altofts Colliery alone is sufficient to prove the case against the Commissioners, for it has been in operation for twenty-one years, and shot-firing has been carried on in it during the whole of that period. If, then, blown-out shots constitute a "not inconsiderable proportion" of the whole, there is a probability amounting to a certainty that such shots must have been fired in Altofts Colliery many times without the results here postulated having been attained until now. The argument as made use of against my own views appears therefore not to be in accordance with ascertained facts.

It may be safely maintained, therefore, that every blown-out shot does not fulfil the whole of the conditions necessary for creating an explosion. For instance, the dust may not be present in sufficient abundance within reach of the flame; it may not be fine enough to ignite at the point where the shot explodes; it may contain too much foreign matter, or be covered with coarse rubbish, or be locally damp; the shot may be too high, or too low, or pointed in an unfavourable direction; the direction and velocity of the passing air-current may exercise some influence on the result; the creation of a nucleus of explosion may in certain cases be facilitated by the previous formation of a cloud of coal-dust to windward, raised by another

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shot (as imagined by the late Prof. Marreco and Mr. D. P. Morrison), or by a fall of roof, a train of mine waggons that has just passed, or any other accidental circumstance, and subsequently carried past the mouth of the shot-hole at the instant the flame issues from it."

Immediately after the passage quoted above the Commissioners proceed to say :—

"The following facts relating to the part played by dust in coal-mine explosions may, however, now be regarded as conclusively established :—

"1. The occurrence of a blown-out shot in working-places *where very highly inflammable coal-dust exists in great abundance*, may, even in the total absence of fire-damp, possibly give rise to violent explosions, or may at any rate be followed by the propagation of flame through very considerable areas, and even by the communication of flame to distant parts of the workings where explosive gas-mixtures, or dust-deposits in association with non-explosive gas-mixtures, exist."

"2. The occurrence of a blown-out shot in localities *where only small proportions of fire-damp* exist in the air *in the presence of even comparatively slightly inflammable or actually non-inflammable, but very fine, dry, and porous dusts* may give rise to explosions the flame from which may reach to distant localities, where either gas accumulations or deposits of inflammable coal-dust may be inflamed, and may extend the disastrous results to other regions."

This has the appearance of conceding all that is asked, but when read in the light of the first quotation it leaves the matter in considerable doubt. Indeed, it was stated at the inquest on Altofts explosion that the proprietors of that colliery had not gathered from the Commissioners' Report that they were running any risk of an explosion, such as the one that happened ; and at the inquest on Elemore explosion, which has been adjourned until the 18th inst., Mr. Lishman, the manager, gave utterance to similar sentiments. Be this as it may, it is obvious that legislative measures ought to be adopted without further delay, with the object of rendering the recurrence of coal-dust explosions impossible for the future. In providing against them it must also be recollected that a local explosion of fire-damp, such as the one which originated Mardy explosion, produces exactly the same result as a blown-out shot fired under the most favourable conditions imaginable.

Cardiff, January 5 W. GALLOWAY

The Cambridge Cholera Fungus

IN your issue of December 23 (p. 171) appears a letter from Dr. E. Klein, in which that gentleman attempts to show that the micro-organisms found by Dr. Graham Brown, Mr. Sherrington, and myself in the substance of the mucous membrane of the small intestine in cases of *Cholera asiatica* are nothing more than "common mould (probably aspergillus)," which has grown on and into the tissue during the process of hardening. We were and are, however, perfectly well acquainted with the fact that imperfectly preserved animal tissues are liable to be invaded by various forms of fungi, and took, therefore, precautions which we believe to be ample to prevent such contamination of our material. Moreover, the presence of the micro-organisms in certain parts of the tissues only, their absence in others or on the surface of the specimens, the fact that their presence in the part is accompanied by anatomical changes which could not have taken place during the process of hardening, and, most of all, the characters of the micro-organisms themselves, render such an hypothesis as that brought forward by Dr. Klein absolutely unacceptable.

It is unnecessary for me to answer all the arguments advanced by Dr. Klein in support of his views on this subject. They prove nothing more than that fungi grow on and in animal tissues which are not adequately preserved—a fact which no one will doubt. That, on the other hand, the micro-organisms found by us are of this nature is a matter which neither Dr. Klein nor any other person who is unacquainted with the facts is in a position to decide. Since a short preliminary account only of the work done by Dr. Graham Brown, Mr. Sherrington, and myself, on the pathology of cholera has as yet been published, Dr. Klein has not before him the facts on which alone a decision of any value is possible. CHARLES ROY

Pathological Laboratory, New Museums, Cambridge
December 30, 1886

An Error in Maxwell's "Electricity and Magnetism"

THE criticism of Mr. McConnel upon Maxwell's derivation of the inductive action of currents from the principle of energy is perfectly correct. It is inconsistent with the experimental facts appealed to by Mr. McConnel and Mr. Maxwell's own treatment of the field as the seat of electro-kinetic energy.

In the excellent treatise of Messrs. Mascart and Joubert, a similar misleading appeal is made to Helmholtz's proof, and I have little doubt that Maxwell has correctly stated it. I should be inclined to think that the existence of the energy of the field was not distinctly present to Helmholtz's mind.

Maxwell, as is well known, by an ingenious application of Lagrange's equations of motion, proves that, in the case of two currents, this electro-kinetic energy T_e is given by the equation—

$$T_e = \tfrac{1}{2}(L_1 i_1^2 + M i_1 i_2 + L_2 i_2^2),$$

where $M = \int\int \frac{\cos \epsilon}{r} ds ds'$ taken round both circuits, and L_1 and L_2 are similar expressions for the separate circuits.

I believe, though I dare not trespass upon your space to give the reasoning *in extenso*, that this result may be obtained somewhat more simply and without the use of the Lagrangean equations, a treatment which has the disadvantage of assuming the electric co-ordinates y_1 and y_2, the currents being $\dot{y}_1$ and $\dot{y}_2$ Then the equation of energy becomes

$$A_1 i_1 + A_2 i_2 = \frac{dT_e}{dt} + \frac{dT_m}{dt} + R_1 i_1^2 + R_2 i_2^2,$$

where T_m is material kinetic energy, and $\frac{dT_m}{dt} = i_1 i_2 \frac{dM}{dt}$ supposing the circuits rigid. Therefore

$$A_1 i_1 + A_2 i_2$$
$$= i_1 \left\{ \frac{d}{dt}(L_1 i_1 + M i_2) + R_1 i_1 \right\} + i_2 \left\{ \frac{d}{dt}(L_2 i_2 + M i_1) + R_2 i_2 \right\},$$

reducing to Mr. McConnel's equation, when the currents are constant.

In the case of two circuits thus moving in connection with their batteries we may infer that A_1 and A_2 must be such functions of i_1 and i_2, and the coefficients of configuration, that, when the suffixes are interchanged in the expression for A_1, that for A_2 must result, and *vice versâ*. If this be so, then the aforesaid equation necessitates the separate equations—

$$A_1 = \frac{d}{dt}(L_1 i_1 + M i_2) + R_1 i_1 \;;$$

$$A_2 = \frac{d}{dt}(L_2 i_2 + M i_1) + R_2 i_2.$$

Or Maxwell's equations are obtained without the use of Lagrange. HENRY W. WATSON

Berkeswell Rectory, near Coventry

The Manipulation of Glass containing Lead

IN a note on this subject in NATURE (Dec. 16, p. 150), Mr. H. G. Madan has made a suggestion which is likely to be very valuable to those who require to manipulate "combustion-tubing" before the blow-pipe. But, in proposing the employment of oxygen in place of air to produce flames for heating glass containing lead, Mr. Madan introduces a refinement which is unnecessary ; for lead-glass may be quite as easily manipulated in flames produced by plain air and gas as soda-glass itself. The pointed flame should be employed for small objects, and the oxidising brush-flame in the case of larger objects. By the oxidising brush-flame, however, I do not mean the brush-flame as ordinarily employed, but one to which the air is supplied liberally through an air-tube without any contraction at its end, and at a steady pressure from a good blower ; care being taken, on the other hand, not to introduce such an excess of air as to reduce the temperature of the flame.

In his note, Mr. Madan quotes me as saying, in the "Methods of Glass-blowing," that the reduction of lead-glass may be prevented or remedied by holding the glass a little in front of the visible flame, with the comment that there is hardly enough heat in that region to do all that is required in the manipulation

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